

Comment on *Acarus putrescentiae* Schrank, 1781 (currently *Tyrophagus putrescentiae*; Acariformes, ACARIDAE): proposed conservation of usage by designation of a replacement neotype

(Case 3501; see BZN 67: 24–27; 71: 99–102)

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This is a response to Fan & Zhang's (BZN 71: 99–102) comment on Case 3501, which involves a conflict between two fundamental principles of zoological nomenclature as embodied in the Code: the Principle of Priority and the need for stability in the use of names. The Introduction of the current Code states, '...the Code recognizes that the rigid application of the Principle of Priority may, in certain cases, upset a long-accepted name in its accustomed meaning through the validation of a little-known, or even long-forgotten name. Therefore the rules must enable the Principle of Priority to be set aside on occasions when its application would be destructive of stability or universality, or would cause confusion'.

Here we provide a brief overview of the case, a discussion of the alternative solution (Fan & Zhang, BZN 71: 99–102), evaluate the reaction of the scientific community, and then give a detailed response to Fan & Zhang's criticism. Throughout the text, we use the following conventions: *Tyrophagus putrescentiae* (common species, = *communis* sensu Fan & Zhang, 2007b) and *Tyrophagus fanetzhangorum* (rare species, = *putrescentiae* sensu Fan & Zhang, 2007b). These two species are genetically distinct and can be easily separated by morphology (Fan & Zhang, 2007; Klimov & OConnor, 2009) and DNA sequences (Beroiz et al., 2014; Klimov & OConnor, 2009).

Case overview

During the course of a revision of Australasian species of the mite genus *Tyrophagus* Oudemans, 1924, Fan & Zhang (2007b) discovered that two distinct morphospecies had been confused under the name, *Tyrophagus putrescentiae* (Schrank, 1781), an extremely common, cosmopolitan mite of considerable medical and agricultural importance. In their material examined from the region and elsewhere, one of the forms was considered 'common' and the other 'rare'. They determined that the neotype designated for this species by Robertson (1959) and validated by Opinion 1298 (BZN 42: 124–126 (June 1985)) does not correspond to the commoner species but to the rarer species. Strictly applying the Principle of Priority, Fan & Zhang (2007b) applied the name '*Tyrophagus putrescentiae*' to the rare species. Those authors did not follow the provision of Article 75.6, which states, 'When an author discovers that the existing name-bearing type of a nominal species-group taxon is not in taxonomic accord with the prevailing usage of names and stability or universality

is threatened thereby, he or she should maintain prevailing usage [Art. 82] and request the Commission to set aside under its plenary power [Art. 81] the existing name-bearing type and designate a neotype.’ Note that we did not suggest that Robertson’s original neotype designation is invalid under Article 75.7 (new in the 4th edition of the Code) because it does appear to meet all the qualifying conditions of the current edition for such designations.

The action of Fan & Zhang (2007b) created nomenclatorial instability because it required that the name of the common species be changed. Based on our study of taxa with existing types (Klimov & OConnor, 2009), there are several available names for the common species. Among these, *T. americanus*, *T. breviceps*, and *T. cocciphilus*, are the oldest names proposed in the same post-1900 publication (Banks, 1906). There are three species described before 1899 and included in the synonymy of *T. putrescentiae* by Robertson (1959) for which types could not be located (*Coelognathus morsitans* Hessling, 1852, *Tyroglyphus lintneri* Osborn, 1893 and *Tyroglyphus ananas* Tryon, 1898). Because these latter names meet the criteria of Article 23.9, they would not be given precedence over Banks’ (1906) names. Because the common species has a long taxonomic history, types of these or additional possibly synonymous taxa may be discovered in the future. This poses a great nomenclatorial challenge – choosing the oldest available synonym for the common species in a way that prevents further changes of the name due to the possibility of discovery of additional historical types.

To maintain the stability of nomenclature, we petitioned the Commission, under Article 75.6, to set aside the existing neotype for *Acarus putrescentiae* Schrank, 1781, and designate a replacement neotype that is consistent with the prevailing usage of the name. Approval of this petition will maintain nomenclatorial stability because there would be no need to change names and will provide a reasonable, although inherently probabilistic, legacy for taxonomic and faunistic works published before 2007. Thus, we propose a conservative approach that does not depend on the outcome of future work and which provides both stability and legacy.

Fan & Zhang’s solution

In their comments to our Petition, Fan & Zhang (BZN 71:102) proposed another solution: ‘Nomenclaturally, the proposers of case 3501 can easily solve the taxonomic problem by synonymizing *T. communis* with a senior name’. This, unfortunately, would not solve the central problem of our petition because the nomenclatorial instability for the common species is linked to the uncertainties associated with the historical types / descriptions (see above). Treatment of *T. communis* (a junior synonym) is a minor issue here.

If Fan & Zhang’s solution were adopted, then the following situation would arise: (i) the common species (*T. putrescentiae*, as understood by the majority of authors) would have no accepted name pending a large-scale taxonomic revision of historical types of taxa described between 1900 and 1906; but (ii) many researchers and public databases (e.g. GenBank) would be prompted to change the name of the common species to *T. communis* (which is an invalid name); and (iii) after a comprehensive study of historical types and descriptions, the common species would change its name again.

Another disruption associated with Fan & Zhang’s solution is the need to change, possibly multiple times, acronyms for important immunogenic proteins. There are

more than 20 groups of allergens that have a specific nomenclature proposed by the International Union of Immunological Societies (IUIS) Subcommittee of Allergen Nomenclature and Standardization (King et al., 1994). For example, Tyr p 2 and Tyr p 10 are the designations for group 2 and 10 allergens of *Tyrophagus putrescentiae*, respectively. These acronyms are so widespread in immunological literature and bioinformatics databases ('prevailing usage' of the name is most often seen in this context) that changing the species name and, concurrently, the allergen abbreviations will be met with a great reluctance from researchers in the fields of medicine, immunology, and molecular biology.

Reaction of the scientific community

Our petition was published five years ago. Subsequently, it has been possible to evaluate the response of the scientific community to our proposal, i.e. to apply Article 82 and conserve prevailing usage. A search for '*Tyrophagus putrescentiae*' yielded 1,870 results and only 4 results for '*Tyrophagus communis*' (Google Scholar, Dec. 24, 2014, phrase quoted, records published since 2009 filtered). For the four results mentioning *T. communis*, only one record used this name as valid – a Ph.D. thesis written entirely in Chinese. Another search for '*Tyrophagus communis*' in the Zoological Record database retrieved a single paper using *T. communis* as a valid name (Cotter et al., 2011). There were no *T. communis* records in the GenBank taxonomy or sequence databases, but for *T. putrescentiae* there were 751 DNA and 1,234 EST sequences (all can be unambiguously attributed to this species).

It is very unlikely that a substantial number of the 1,870 records citing *T. putrescentiae* may refer to Fan & Zhang's concept (i.e. the rare species), because it would be accompanied by an explicit citation. Google Scholar gives only 22 citations of Fan & Zhang's monograph since 2009. To illustrate our point further, we give a list of 16 papers and 1 taxonomic monograph published after 2009 that are in favor of preserving the name of the common species (Beroiz et al., 2014; Eaton & Kells, 2009; Freitag & Kells, 2013; Frost et al., 2010; Guanilo et al., 2012; Hubert et al., 2009, 2012a, 2012b, 2013; Kucerova & Stejskal, 2009; Murillo et al., 2013; Palyvos & Emmanouel, 2011; Que et al., 2014; Solarz, 2012; Stara et al., 2014; Stara et al., 2011; Torre Santana & Rodríguez Castro, 2010). These articles either explicitly cited the taxonomic problem, or deposited sequences matching *Tyrophagus putrescentiae*, or there was secondary evidence (e.g. Freitag cited in a Ph.D. thesis but not in the related paper; Hubert and his group based their work on a culture with known identity).

The evidence given above indicates that the scientific community consistently followed our citation of Article 82 to maintain the prevailing usage of *Tyrophagus putrescentiae* and not to change it to *T. communis* (or any other name).

Fan & Zhang's criticism of the Case

Below we number all sections as they appeared in Fan & Zhang's comment and reply to their specific critiques.

(1) 'Lack of understanding of the Code and disregard of its rules by authors of Case 3501'.

1.1. Article 75.4 was cited as 'violated'. This Article states 'The first neotype designation . . . is valid and no subsequent designation, *except one made by the*

Commission . . . , has any validity . . . [italics ours]. Our petition specifically seeks such a subsequent neotype designation under the plenary power of the Commission directly in accordance with Article 75.4.

1.2. Article 80.9 was cited as ‘violated’. This Article states ‘Previous decisions of the Commission. No ruling given by the Commission . . . is to be set aside without the consent of the Commission’. Specifically in accordance with Article 80.9, we seek the consent of the Commission to set aside its previous decision on the neotype of *Acarus putrescentiae*.

1.3. Fan & Zhang also criticized us for designating neoparatypes (p. 99). However, our petition does not designate neoparatypes nor even mention them. Neoparatypes are not regulated by the Code, hence are irrelevant to the issue.

(2) ‘Lack of sufficient evidence for ‘prevailing usage’ in Case 3501’.

To determine prevailing usage of the name *T. putrescentiae*, in 2007–2009 we conducted a survey of works using this name and published over the preceding 20 years, requesting authors of 49 of these works to allow us to examine their specimens. Of these, the authors of 31 works sent their specimens. Other authors reported that their vouchers were lost or not saved, and some authors responded that they no longer worked in acarology or did not respond at all. Thus, it was impossible to conduct an exhaustive study (as suggested by Fan & Zhang). Because all of these published works were treated equally (i.e. no selection bias), our survey is both representative and instructive in estimating prevailing usage. The result: 30 works (14 authors) actually published on *T. putrescentiae* and one work actually on *T. fanetzhangorum*.

Below we discuss more specific criticisms presented in this section.

2.1. ‘They did not mention that Fan & Zhang (2007b) examined some 60 [actually only 26 listed] specimens [of the rare species] available to them’ (p. 100) and the lengthy list of distributions of both species on pp. 100–101. This criticism is irrelevant to the Code definition of prevailing usage. Prevailing usage is defined as usage by ‘a substantial majority of the most recent authors’, not based on specimens examined or geographic distribution.

2.2. ‘For hundreds of studies on *T. putrescentiae*, a sample of 31 published studies by 14 authors is a very small minority’ (p. 100). The Glossary of the Code defines prevailing usage of a name as: ‘that usage of the name which is adopted by at least a substantial majority of the most recent authors concerned with the relevant taxon, irrespective of how long ago their work was published’. We do not consider our verifying the usage of *T. putrescentiae* in 30 of 31 cases in the past 20 years out of 49 attempts as representing ‘a very small minority’.

2.3. Fan & Zhang argue that we did not demonstrate prevailing usage because our survey was ‘biased’ by papers based on ‘laboratory-reared material’. This is not a bias but a reflection of the real situation. *T. putrescentiae* is widely used in various molecular, medical, immunological, and pest-control studies that rely on pure cultures reared in a standardized manner in a laboratory (rather than collected from environmental samples). Not surprisingly, usage of this name in these papers is much more frequent than in taxonomic ones. We note that this argument is somehow ironic because Fan & Zhang’s name ‘*T. communis*’ is also based on specimens from a laboratory culture.

2.4. Although the issue of whether *T. putrescentiae* is more common in nature than *T. fanetzhangorum* is irrelevant to determining prevailing usage, Fan & Zhang challenge this, unfortunately providing no supporting evidence. Because in the text we use ‘common’ for *T. putrescentiae* and ‘rare’ for *T. fanetzhangorum*, we give reasons for such definitions here: (i) in our collection (UMMZ) there are nine 100-slide boxes with *T. putrescentiae* and only one box (two collection localities, fewer than 20 slides) for *T. fanetzhangorum*; (ii) the specimen count for *T. putrescentiae* (= *communis*) is 355+ as against 26 for *T. fanetzhangorum* in the *Tyrophagus* monograph (Fan & Zhang, 2007); (iii) *T. putrescentiae* was included in the latest key to stored product and house dust mites, but *T. fanetzhangorum* was not, despite its author’s awareness of the name (Solarz, 2012), indicating its low or negligible prevalence as compared to *T. putrescentiae*.

In conclusion, we do not think our sample was biased or insufficient regarding the recent published literature, which is the point of contention with respect to Code application. It was not our point that our *T. putrescentiae* was more ‘common’ in nature (although evidence suggests it is), only that we verified that the name was applied to the single (‘common’) species by the overwhelming majority of recent authors, thus constituting prevailing usage.

(3) ‘Inaccurate perceptions of presumed disruption to stability by authors of Case 3501’. The case of *Varroa destructor* versus *Varroa jacobsoni* was given as an example where a community accepted a name change following revision of species concepts. However, in this example both names are valid. In our case, one of the names, *T. communis*, is invalid (a junior synonym), and there is uncertainty with respect to choosing the oldest available junior synonym of (the common) *T. putrescentiae* should that name be applied to the rare species. Moreover, unlike the case of the *Varroa* species, which parasitize different host bees in different geographic regions, the identity of the common and rare species cannot be deduced from their habitats but only from their morphologies or DNA sequences. Thus, changing the concept of *T. putrescentiae* will create a large-scale nomenclatorial conundrum and result in great instability (see the section ‘Fan & Zhang’s solution’ above).

(4) ‘Misinterpretations by the authors of this case of the work of Fan & Zhang (2007a, b)’

Here Fan & Zhang refer to ‘misinterpretations’ of their two published works, however, in their comment specifically referring to the names *T. americanus*, *T. breviceps*, *T. cocciphilus*, and *T. castellanii*, they refer to unpublished data (without specifying their nature). The nature of their unpublished evidence can be seen from Dr. Fan’s message distributed to the Acarology listserv on Jul 12 2009: ‘According to our study of the specimens (*Tyroglyphus americanus* Banks, 1906; *T. breviceps* Banks, 1906; *T. cocciphilus* Banks, 1906; *T. sacchari* Banks, 1906 [sic], the conditions of these specimens were not ideal, even after re-mounting), at that moment we could not conclude that they were conspecific; therefore, we retained names. Therefore, it is better to retain these names until we are capable of resolving the problems.’

The type series of these species were originally mounted in Canada balsam (a hydrophobic medium with a poor refractive index for mites), and for that reason, it was impossible to study them in the detail necessary in that state. Our subsequent examination of these specimens indicated someone had attempted to remount them in an aqueous medium (Hoyer’s), but the attempt resulted in damage to the

specimens. However, as we indicated in our publication (Klimov & OConnor, 2009a), there were multiple syntype slides, and we were able to dissolve the original mounting medium of representatives by consecutive xylene/ethanol washes and then successfully remount them in Hoyer's. This produced excellent specimens, with all diagnostic characters clearly visible. Based on the remounted specimens, we designated lectotypes, synonymized *T. americanus*, *T. breviceps*, and *T. cocciphilus* with *T. putrescentiae*, and provided microscope photographs illustrating the diagnostic characters in the remounted specimens (Klimov & OConnor 2009a; BZN 67: 24–27). Regrettably, Fan & Zhang (BZN 71: 99–102) did not comment on this critical synonymy (likewise our synonymy of other taxa, except for *T. amboinensis*) that make *T. communis* Fan & Zhang a junior synonym and thus invalid.

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Comment on *Saturnia canningi* Hutton, 1859 (currently *Samia canningi*; Insecta, Lepidoptera, SATURNIIDAE): proposed conservation
(Case 3638; see BZN 70: 229–233)

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All points mentioned in Case 3638 by Peigler & Luikham can be completely accepted. As co-author of Peigler's revision of the genus *Samia* the cited literature and facts are well-known to me, and I think that nothing else needs to be added.

I would advise the Commission to rule in this case exactly as proposed by the authors.

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In my opinion, Case 3638 should be supported and accepted. The authors have explained the Case based on a wealth of information. It would be very helpful to have two different valid names to address the wild population of the Himalaya as *canningi* and the domesticated 'race' as *ricini* separately without any potential danger of confusing them and in concordance with the overwhelming majority of the literature.

The Commission should rule in favour of the points proposed by the authors of this case.

**Comments on *Antheraea roylei* Moore, 1859 (Insecta, Lepidoptera, SATURNIIDAE):
proposed conservation**

(Case 3635; see BZN 70: 221–228)

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Case 3635 should be rejected by the ICZN as there is no reason to rule on the availability of the two involved taxa, *Antheraea roylei* Moore, 1859 and *Bombyx* (*Saturnia*) *pernyi* Guérin-Méneville, 1855 (now in *Antheraea*). The authors act on the incorrect assumption that both taxa are conspecific, and that *Antheraea pernyi* would be the domesticated form of its wild progenitor *Antheraea roylei*. In the following points (with similar order and numbering as in the original Case 3635) it is shown that both taxa are not conspecific.

1. The name *A. roylei* was used several times since its misspelling in the original description by a few German authors in the late 20th and early 21st century (e.g. Paukstadt et al., 2000; Paukstadt & Paukstadt, 2008; Brechlin & Paukstadt, 2010), but nevertheless, with the argumentation of Nässig & Holloway (2010) and the continued citation as *A. roylei* in most of the publications since its original description, it becomes clear that *A. roylei* should be the nomen protectum and *A. roylei* the nomen oblitum. In total, there were less than 10 authors citing the taxon as *A. roylei*, so the conditions of Article 23.9.1.2 are fulfilled anyway.

3. The argumentation by Peigler (2012) and paragraph 3 of Case 3635, that both *A. roylei* and *A. pernyi* are the same biological species, cannot be followed. The true wild *A. roylei* occurs on the southern slopes of the Himalayas in countries such as Afghanistan, Pakistan, Nepal, Bhutan and India (Sikkim, Assam, Arunachal Pradesh), and *A. pernyi* occurs well-separated from this species in the wild in most parts of PR China, Taiwan, Mongolia, SE Russia and Korea. In addition to *A. roylei* and *A. pernyi*, there exist some further related species described from further south (e.g. *A. korintjiana* Bouvier, 1928, *A. lampei* Nässig & Holloway, 1989) which were not mentioned in this case at all. Of course there can always occur escapees from sericultural colonies, or material in collections derived from dealers who sold specimens from silk farms, which may be the case for specimens from northern China, far east Russia or Japan, and which can, for example, be determined by fat bodies of females, which under normal conditions would not be attracted to light traps; however, in material from different provinces in China, collected by professional and experienced entomologists during the last two decades, a small number of males of *A. pernyi* were always found among other typically wild species in their natural habitats. Introduced populations of *A. pernyi* on Mallorca, Balearic Islands, Spain, did not last only 'less than 10 years' after their introduction in 1881 but still occur uncommonly in the wild there; a review with actual records for the last 50 years was published only recently by Pinya et al. (2013). Of course, populations of *A. pernyi* introduced in Romania, Ukraine or Japan for silk production did not persist for long, although records are known for Japan (Kishida, 2012), but the extinction of populations only in probably inadequate climate zones is no reason to argue for the synonymy of *A. pernyi* and *A. roylei*. Also the fact that Chinese authors regularly assigned the name *A. pernyi* to specimens collected in China, while in other countries

such as India, Nepal, Thailand etc. local writers 'determined' their records as *A. roylei*, does not mean anything. As we know, especially in Indian and Chinese literature, many misidentifications of even well-known SATURNIIDAE species exist, so published misidentifications are no reason to argue for conspecificity. Finally, the source of the material used in Jolly's studies (Jolly et al., 1979) on offspring of multiple generations is somewhat tentative, and it is not certain that he really used true *A. pernyi* for his hybrids.

4. Specimens of *A. pernyi* and *A. roylei* are easily separated from each other by the different form of the forewings in males (more rounded in *A. pernyi*, more elongate and slender in *A. roylei*), and in fact do have 'consistent wing pattern characters to separate them': *A. pernyi* is always of a much lighter, more ochreous colour and has larger ocelli on its wings, especially in the males. Larvae differ in details, just as male genitalia do in series. These are additional characters to separate both taxa, neglected by Peigler & Chutia, which goes along with different cocoon structures and chromosome numbers of both taxa, as mentioned in paragraph 4 of Case 3635. All this is further supported by results of DNA barcoding of the COI marker gene of a larger series of fresh material which was not used in the studies of Peigler & Chutia (unpublished data).

5. There is no evidence that *A. roylei* represents the wild form while *A. pernyi* exists only in captive colonies. *A. roylei* exists in the wild as mentioned above, but is also used in India for Tussah sericulture; *A. pernyi* is a widespread species which occurs in the wild in most southern and central parts of PR China and in Taiwan; records mentioned in Case 3635 as *A. roylei* for the Chinese provinces of Fujian, Jiangxi, Hunan, Sichuan, Yunnan, Guangdong, Guangxi, and Shaanxi are in fact wild collected specimens of *A. pernyi*. Records from northern China, Korea, Russia and Japan may be in part based on escapees from silk productions, but at least for far east Russia and Korea some wild records exist, and it is an incorrect assumption that *A. pernyi* does not have wild populations there. Of course it was introduced by man into Japan.

6. While Peigler & Chutia are correct with their statement that both *Bombyx mori* and *Samia ricini* are entirely of sericultural origin and do not exist in nature (although sometimes some males of *S. ricini* escape from silk farms in Thailand and Vietnam and can be collected around lights), this is not true for *A. pernyi* in southern China and Taiwan where the species occurs in the wild and is a typical faunal element, although also used for silk production. Both *B. mori* and *S. ricini* do not fly regularly, but *A. pernyi* flies very well.

7. The argumentation by Peigler & Chutia is not correct: for the entire Chinese area both wild and sericultural populations are *A. pernyi* and, to my knowledge with respect to sericulture, never *A. roylei*, while it is the opposite in India. There is not much confusion even in countries south of China if this fact can be accepted, but there are lots of misidentifications in especially sericultural literature which must be corrected in a revisional work. Even from their handling of other involved SATURNIIDAE species, all cited Indian works are taxonomically irrelevant as they are not based on modern taxonomy but still handle species' identities on the level of Hampson (1893); also Pinratana & Lampe (1990) and Allen (1993) are out of date with their identifications.

8. The mentioned publications of Chinese works contain many misidentifications as well, and in most publications determinations are just copied from previous

literature, often including their misspellings. Nevertheless, Chinese authors as cited in the Case used the right name *A. pernyi* in general for Chinese populations, and only the taxon *A. pernyi yunnanensis* Zhu & Wang, 1993 (which in fact is a synonym of *A. platessa* Rothschild, 1903) leads to some confusion. In general there is no need to distinguish in this case between wild and sericultural populations of *A. pernyi*. The cited publications of Mell (1939) and Sonan (1937) both contain misidentifications of *A. pernyi* as *A. roylei*.

9. All cited publications ('major taxonomic catalogues and monographs') used the correct classification with *A. pernyi* originating from China and *A. roylei* being the sub-Himalayan species, so there is no reason at all to change this at the moment. Only a revision of the entire species group would allow a better overview, with classification of further involved taxa mainly with more southern distribution.

10. Also the additional publications used the correct classification with two different species, but not in the sense it is now proposed in this Case.

11. Again, it is somewhat tentative to discuss the hybrids; the source of the '*A. pernyi*'-stock used is uncertain, and it is not in fact clear whether authors based their work on true *A. roylei* specimens.

12. As there is no need to synonymise *A. pernyi* with *A. roylei*, this entire matter can be ignored. Of course the wild populations of *A. pernyi* in China and their habitats should be protected.

13. The authors are correct to ask for the continuing use of the two names *A. pernyi* and *A. roylei* in future, but they are not correct with their assumption that *A. pernyi* does not have feral populations in China and elsewhere. Judging only from the fact that an insect is used in sericulture does not mean that the wild progenitor would need to have a different name from its domesticated relative. A similar case would exist for *Antheraea assamensis* (Helfer, 1837) which is used in Assamese sericulture for the production of Muga silk; probably nobody would propose the use of a different name for its wild progenitor which is widespread in the sub-Himalayan area.

14. Judging from all those arguments, it becomes clear that *Antheraea roylei* and *Antheraea pernyi* are different species; therefore no synonymy exists, and no conservation of the name for a domesticated race is necessary here at all. Case 3635 should thus be rejected.

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In my opinion, Case 3635 should be rejected by the Commission as there is no reason to rule on the availability of the names of the two taxa involved, *Antheraea roylei* Moore, 1859, and *Bombyx (Saturnia) pernyi* Guérin-Méneville, 1855 (now in *Antheraea*). The authors wrote their suggestion based on the incorrect assumption that both taxa are conspecific, and that *Antheraea pernyi* would be the domesticated form of its wild progenitor *Antheraea roylei*. This interpretation is mainly based on erroneous and misleading literature (especially from the field of sericulture) and insufficient wild-collected material from all over Asia being available to the two authors.

As has also been demonstrated at length in the comment on Case 3635 by S. Naumann (BZN, this issue), it is evident from external and internal morphology of adult moths as well as morphology of preimaginal instars, from biogeographical considerations (in combination with other species of this species-group from SE Asia) and also from the DNA COI barcode sequences (all these characters studied on a wealth of material collected in the wild at very many localities in Asia) that both taxa are not conspecific (unpublished data). There are indeed sericultural forms, but in the present species-group these are not yet selected into domesticated 'races' unable to survive in the wild, as has been done with especially *Bombyx morilmandarina* and (in part) *Samia ricini/canningi*. The case of *Antheraea pernyi* and *A. roylei* is different from these other examples in sericulture, and the other two cases of established sericulture in *Antheraea* species probably follow the same pattern.

Case 3635 should thus be rejected, as the two taxa are not conspecific but are clearly separate species.

Comment on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published

(Case 3601; see BZN 70: 234–237; 71: 30–38, 133–135, 181–182, 252–253)

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The comments by Kaiser (BZN 71: 30–35), Schleip (BZN 71: 35–36), Wüster et al. (BZN 71: 37–38) and Thomson (BZN 71: 133–135) provide no hard evidence to rebut any element of Case 3601 as originally published in BZN. Correspondents on Taxacom and the ICZN list online have correctly dismissed the claims of these authors as ‘bluster’. Thorpe (in litt.) added that the comments submitted by Hoser’s enemies on Case 3601 were absolutely laughable in terms of the Code, and this view was repeated in similar words by Dubois, Wellington and others (in litt. 2014). Furthermore all claims raised by Kaiser, Schleip, Wüster et al. and Thomson, merely repeated earlier discredited claims of Kaiser (2013) and Kaiser et al. (2013). These were rebutted in detail by Hoser (2012a) (regarding the *Spracklandus* matter), Hoser (2012b) and Dubois (in litt.) in relation to other issues alleged by the group. However I here deal briefly with some of the points for purpose of further rebuttal and for discussion in BZN.

Claims by Kaiser (BZN 71: 133–135) that the Hoser works have been criticized by others have no bearing on the nomenclature and the case for ICZN confirmation of the nomenclatural availability within the Code of the name *Spracklandus* Hoser, 2009. In any event, the criticism of scientific papers is normal scientific discourse, even if labelled by critics as ‘unscientific’. Kaiser has not in fact produced any evidence to suggest that Australasian Journal of Herpetology (AJH) Issue 7 did not comply with Article 8.1.3 of the Code. The distributed issue of AJH Issue 7 is no different from other acknowledged Code-compliant papers published daily. It was published in print with ink on paper in numerous durable copies. An alleged printing defect in terms of printing quality control as suggested by Kaiser (2014) does not in any way make AJH Issue 7 invalid under the Code or Article 8 of the Code. Kaiser’s statement that ‘I have seen no proof that there were ever more than a handful of copies produced’ is meaningless. Absence of evidence is not evidence of absence. Furthermore at no stage have Kaiser, Wallach, Wüster, Broadley or Schleip asked the logical question of me as to where and by whom original copies were distributed.

Hoser (2012a) stated that ‘All issues of AJH were published in hard copy (over 100 originals of each) and later online, being posted online on average 10 days after the print copies were first received and distributed, by which stage receipts from recipients had been received and archived.’ Kaiser, Schleip or Wüster et al. have never produced any evidence to contradict this obvious fact or properly sought contradictory evidence. Kaiser (2014b) used four arguments to allege that AJH was in violation of Article 8.1.3 of the Code. These arguments are refuted in detail below.

(1) It was published in an edition, in the usual meaning and understanding of this word, and even cited as such by his close colleagues (Wallach et al., 2009, p. 34).

- (2) There is uncontradicted evidence that numerous copies were made (e.g. Hoser, 2012a). The only evidence provided by Kaiser, Wallach, Wüster, Broadley or Schleip is a statement that they did not make proper or reasonable enquiries as to where copies were distributed. This included failing to check the most likely repositories, such as Zoological Record, as specified in the Code (Recommendation 8A) or persons named in the relevant papers.
- (3) The original copies were all identical in words, fonts, pagination, margins and all other relevant details.
- (4) The copies were 'durable' in the commonly accepted meaning of the word, including being printed on high quality white gloss paper (superior to that used by most other published journals) and printed in black ink.

Furthermore, all Kaiser's claims against the method of printing of AJH in terms of potential Code-compliance (repeated in part by Wüster et al., 2014) are in fact rejected by Schleip (BZN 71: 35–36) in his point 5.

Schleip's claim 'The existence of this outlet [AJH] was primarily proclaimed in herpetoculture internet forums, and zoologists unlikely to participate in such forums were widely unaware of its existence (see the Code, Appendix B.8, General Recommendations).' is clearly false. In the pre-checking of Case 3601, the Commission Secretariat independently established that AJH was sent to numerous places including Zoological Record as the most important part of the Code's 'wide dissemination' recommendation (Recommendation 8A). Significantly, Schleip's claim is also refuted by Wüster (in litt. 2009), who wrote 'You have been accused of many things. Lack of dissemination of your articles has not been one of them'. Schleip's claim 'However, on the date [Issue 7 of AJH] was distributed, it was not obtainable by the public' is patently false. AJH was available at all relevant times.

Schleip (BZN 71: 35–36) stated 'In the absence of evidence to the contrary, it is not possible to determine whether or not the copies were printed in accordance with Article 8.1.3 or 'printed on demand'. Kaiser (BZN 71: 30–35) asserted 'I believe this shows that there really was no print run of numerous identical and durable copies'. These and similar statements about the availability of AJH and the relevant website are incorrect. Printed issues have always been offered for sale, the price of each being determined by size. I have often chosen to waive fees to persons requesting issues or photocopies of papers, as was the case with Wallach in 2009. As each issue of the journal was published, the relevant details were added to the relevant parts of the website in accordance with similar practices by publishers of other scientific literature.

Kaiser's (BZN 71: 30–35) prediction of mass disobedience against the Code in the event of a judgement in favour of *Spracklandus* was repeated by Wüster et al. (BZN 71: 37–38), Thomson (BZN 71: 133–135) and Schleip (BZN 71: 31–36), who said 'If the Commission, however, were to vote in favor of Case 3601 and declare the name *Spracklandus* Hoser, 2009 available . . . I predict that the majority of herpetologists will follow the recommendations of Kaiser et al. (2013) and continue to ignore AJH as a reliable source for nomenclatural and taxonomic information'. This is the same prediction made in BZN in relation to the Wells & Wellington papers and names proposed within them made by Stone and others. Stone (1988) wrote: 'If the Commission takes no action with respect to the nomenclature proposed in these publications other scientists may of course choose to ignore that obligation'. King

(1988) made similar comments to those of Kaiser et al. (2013) when he said: 'If [the ICZN] fail to [suppress the works of Wells and Wellington] they will jeopardise the survival of the system of nomenclature which we all use'. Following the Commission's judgement in favour of Wells & Wellington (ICZN, 1991) there was no such mass disobedience against the Code, and in the fullness of time the original Code-compliant names were accepted and widely used (Cogger, 2014a; Shea, in litt.) and the Code survived intact. This usage included the original Code-compliant names being used by authors in favour of the junior synonyms coined by the protesters who had hoped the ICZN would formally suppress the earlier Code-compliant papers (Cogger, 2014a; Shea, in litt.).

Kaiser's claim to represent 'the herpetological community' (as also made by Wüster et al., BZN: 37–38) is false, as demonstrated by Wellington, Wells, and others (in litt. 2013–2014), but is similar to the claims made by those seeking to suppress the Wells & Wellington papers (see ICZN, 1991). Similar claims made by Kaiser in comments on the current case must also be rejected.

Cogger (2014 and in litt.), Dubois, Eipper, Mutton, Shea, Thorpe, Wellington, Wells and many other eminent herpetologists (in litt. 2013–2014) have condemned the Kaiser scheme to replace the names of hundreds of valid taxa with new names. This clearly disproves the claim by Kaiser (2014) that he has broad agreement within the herpetological community for his plan to step outside the Code. In a public online forum Wells told Wüster and Schleip, 'what you and others are doing in this regard is highly contemptuous of the authority of the ICZN'. Thorpe (in litt. 2013) wrote 'At the end of the day, Wolfgang, you are just complaining about the authorship of names which may have to be used as valid . . . complaining that they are not yours (or those of people you choose to consider to be colleagues. . .)'. Shea (in litt.), described Kaiser et al.'s (2013) plan as being 'unworkable'. Eipper (in litt.) noted 'You cannot use a viewpoint to act as a veto to disregard the use of the Code'.

More recently, Schleip (2014) renamed *Leiopython hoseae* Hoser, 2000 with his own name '*L. meridionalis*'. Schleip claimed that Hoser (2000) did not comply with Article 8.1.1 of the code (invoking Kaiser et al. 2013 and Kaiser 2014), which reversed his own opinion expressed in Schleip (2008), Schleip & O'Shea (2010) and even Kaiser et al. (2013), all of whom accepted and used the correct Hoser's (2000) name. Significantly, Schleip (2014) was published in the face of advice by two separate expert reviewers that his paper's claims against Hoser (2000) were false and that he would be acting in contempt of the Code (Shea, Raw, in litt.). Shea formally recommended rejection of the Schleip paper to the editors of Journal of Herpetology. The publication of Schleip (2014) was also condemned by Wellington, Uetz and Cogger (in litt.) within hours of its appearance online. Uetz asked 'How can this go past a reviewer or editor?' In a similar example, Hedges et al. (2014) used the Kaiser 'veto' to overwrite the previously accepted and used *Argyrophis* Gray, 1845 with their own coined name, seriously destabilizing the nomenclature of the blindsnakes.

The claim by Wüster et al. (BZN 71: 37–38) that Hoser had unethically scooped their own allegedly pending work by naming *Spracklandus* is rebutted by Wüster himself (in litt.) where six days after the publication of Hoser (2009), he condemned the taxonomy in that paper to a global audience and added: 'The case for keeping [*Naja*] as a single genus was made by Wüster et al., 2007.' Fry (in litt.) followed this on the same date with 'Wolfgang's 2007 paper already considered the higher order

taxonomy of cobras and quite rightly lumped them into a single genus'. Hoser (2009) had clearly rejected Wüster's own published taxonomy and the appropriate Code-compliant nomenclature of *Spracklandus* Hoser, 2009 followed from this. From the content of Wüster (in litt., 2009) it is clear that Wüster et al. amended their own taxonomic views to align with those of Hoser, well after the publication of Hoser (2009). This means it was not possible for Hoser to have improperly knowingly 'scooped' any work or ideas of Wüster at the time Hoser (2009) was published, as Wüster et al. have more recently alleged.

Case 3601 as originally put by myself should therefore be upheld by the Commission. It is in the interests of long-term nomenclatural stability that the Commission act decisively. It should make a strong statement condemning the actions of Kaiser et al., who have aggressively operated in contempt of the Code. Failure to do so will destabilize taxonomy and nomenclature. The issue is not 'Hoser' but the stability of the Code. The Commission must protect the Code from Kaiser et al. and others who will emulate them to create nomenclatural chaos if their current campaign is successful.

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1. In Case 3601 Raymond Hoser has asked the Commission to validate for the purposes of nomenclature the name *Spracklandus* Hoser, 2009, and ‘the journal in which it was published,’ issue 7 of the *Australasian Journal of Herpetology* (AJH). We note that the entire run of AJH has been written, edited, and published solely by Hoser. Although his requests to the Commission were presented as narrow and, in his words, ‘routine matters,’ we are convinced that they represent an important tipping-point with broad implications of major concern for zoological taxonomy and nomenclature as a whole and, by extension, the greater scientific community. Since Hoser’s actions and works have failed to follow scientific best practices (e.g. Turtle Taxonomy Working Group, 2007, 2014; Kaiser et al., 2013; Kaiser, 2014) and both the Commission’s general Recommendations and Code of Ethics in Appendix A, the global herpetological community has widely rejected his taxonomic decisions and resultant nomenclature. This has unfortunately caused a confusing dual nomenclature to develop in the herpetological community, with most boycotting or ignoring Hoser’s 700+ new names coined in the AJH, while he and a few personal followers actively promote their usage. We believe that suppression of the name *Spracklandus*, and all issues of AJH, is the only effective way to bring this contentious and confusing issue to resolution. The plenary power available under Article 81.1 of the Code exist specifically to allow the Commission to make rulings in individual cases that disturb stability and cause confusion, whether the works are Code-compliant or not. We maintain that it is in the interest of nomenclatural stability, not only for herpetology, but for all of zoological taxonomy, that the plenary power be invoked to declare the works in AJH unavailable, regardless of any narrow interpretation of their technical Code-compliance. We present our arguments for rejection of the validity of AJH in the following commentary. In view of the wide-reaching implications of this case for all of zoology, and reflecting the deep and broad-based community concern over these issues, our contributing authors include 70 global scientific leaders and accomplished amateurs from a wide variety of zoological disciplines.

2. When it comes to identifying and naming taxa, herpetology has embraced the use of possibilities created by emerging technologies in desktop-publishing, open-source internet-based publishing, searchable online digital libraries and databases, and internet search engines (e.g. Chelonian Conservation and Biology, www.chelonian.org; Herpetological Conservation and Biology, www.herpconbio.org; Amphibian Species of the World, <http://research.amnh.org/herpetology/amphibia/index.html>; The Reptile Database, www.reptile-database.org; Turtle Taxonomy Working Group, www.iucn-tftsg.org/checklist). Along with this acceleration and expansion of scientific communication, the last 20 years have witnessed unprecedented increases in the knowledge of reptile phylogeny and species

diversity, triggered by the ever-increasing application of molecular data, new analytical tools and conceptual advances, with the resulting taxonomic adjustments affecting most groups. Unfortunately, these advances in online informatics and desktop publishing have also created easier mechanisms for some individuals to bypass scientific publishing conventions in order to create self-published, poorly justified, and questionable nomenclatural and taxonomic acts, without the quality control of peer-review and editorial oversight. Case 3601, concerning the validity of the name *Spracklandus* and issue 7 of AJH, the journal in which it was presented, illustrates the potential of technological progress and internet-based data-mining by some individuals to enable the rapid production and wide distribution of this type of work, and with it the accelerating threats of destabilization and confusion that nomenclatural and taxonomic systems are facing as a result. Although rogue taxonomic journals such as AJH have appeared in the past and continue to appear (see Raghavan et al., 2014), it is our contention that it is incumbent on the scientific community and the Commission (ICZN) to protect and defend the integrity of nomenclatural taxonomy and the scientific process, lest both science and the ICZN itself fall victim to the destabilizing impact to nomenclature and taxonomy of the kind generated by individuals such as the originator of Case 3601.

3. Poor quality output is unacceptable in all fields of science, but it is particularly deplorable in taxonomy because it creates persistent nomenclatural instability and confusion, debases proper taxonomic and nomenclatural work, and bypasses accepted and established community standards for scientific inquiry and process. Kaiser et al. (2013) appropriately criticized this type of output, but in response to Kaiser and co-authors, Hoser (2013b) disingenuously and abrasively attempted to justify his own work and defend his actions of naming un-named phylogenetic clades identified in the works of other authors, while at the same time engaging in extensive personal defamatory rhetoric (e.g. Hoser 2013b:12, 15). We fear that unless the Commission addresses this type of divisive approach to taxonomy and nomenclature, and its potential ramifications, by setting a clear precedent to stabilize herpetological nomenclature, comparable practices are likely to surface in other branches of zoological taxonomy. If left unaddressed or validated, further destabilization and confusion are likely to develop, and the ICZN would then find itself facing an onslaught of analogous problems.

4. Case 3601 attempts to confirm the availability of the name *Spracklandus* and validate for the purposes of nomenclature issue number 7 of AJH. By implication, this could be misconstrued as validating the entire run of AJH as an acceptable medium for nomenclatural acts, and with it the many names (currently over 700, at various taxonomic levels) that Hoser created in isolation from (and frequently in conflict with) the global herpetological community. In addition, it would implicitly validate what we consider to be Hoser's disregard for proper scientific conduct and process. While we understand that any vote by the Commission is intended to be narrow in its application (i.e. covering only the specifics of the individual case) and not precedent-setting, we are certain that, given his long and public history of self-promotion, Hoser will interpret a decision in his favor as precedent-setting and comport himself accordingly, including the likely possibility of hundreds more requests to the Commission for validation of his many names. It is for this reason that our comment deliberately reaches beyond the specifics of Case 3601: to pre-empt

claims about the relationship between the Commission and herpetological taxonomists that manipulate the intent of a decision on Case 3601 and do lasting damage to the Commission and its effectiveness in science.

5. Hoser's output threatens to undermine the entire Code-compliant system that underlies nomenclatural stability (Kaiser et al., 2013; Thomson, BZN 71: 133–135). Having already impugned the scientific reputation and credibility of individual taxonomists (e.g. Hoser, 2009, pp. 16–19; Hoser, 2013b pp. 12, 15) and undermined the taxonomic profession itself (by self-publishing in a journal with no evidence of independent peer review), Hoser has triggered unprecedented community reaction and rejection (Kaiser et al., 2013; Thomson, BZN 71: 133–135). The herpetological community has expressed, justified, and implemented its intent to reject usage of Hoser names (e.g. Bates et al., 2013; Measey, 2013; Reynolds et al., 2014), but Hoser has continued to promote his alternative nomenclature and promulgated ever more names, with more issues of the AJH introducing many more new names posted in August 2014. Recent pending ICZN applications regarding Hoser names (Cases 3647 and 3648) and his comments on Taxacom and the ICZN-list online discussion forum have also indicated that his application regarding *Spracklandus* will not be an isolated case.

6. One of the most difficult situations arising from this scenario is the emergence of mutually exclusive, conflicting dual nomenclatures. One is based on accepted scientific principles to ascertain that the production of peer-reviewed taxonomy and nomenclatural acts is based on rigorous and focused analysis and a shared Code of Ethics; this is the method widely supported by the global herpetological and other taxonomic communities. The other is produced in isolation and based largely on apparent misappropriation and misrepresentation of others' work, or occasionally on baseless conjecture, without any notable adherence to acceptable scientific rigor or ethical principles. The ICZN is already aware of specific examples (see Thomson, BZN 71: 133–135), including Hoser's recent pre-emptive but technically flawed attempt to name *Macrochelys* taxa under active study by others (Roman et al., 1999; Echelle et al., 2010; Hoser, 2013a; Turtle Taxonomy Working Group, 2014; Thomas et al., 2014), in egregious disregard of Appendix A:2 of the Code. Developments of the most recent past have shown that the herpetological community is determined to uphold a boycott of Hoser names, and at least the *African Journal of Herpetology* has published an editorial that formalizes this boycott (Measey, 2013). Without action by the ICZN in opposition to Case 3601, we fear that dual nomenclature will be a perpetual problem for herpetological taxonomy.

7. An example of developing dual nomenclature is Hoser's attempted resurrection of three rattlesnake genera (*Aechmophrys*, *Caudisona*, and *Uropsophus*) from the synonymy of *Crotalus*, along with the description of new genera and subgenera (Hoser, 2009; Wüster & Bérnils, 2011). No data were presented to support these proposed changes, and Zaher et al. (2009) recommended that they not be followed. Despite this, the Brazilian Society of Herpetology unfortunately adopted these changes in their annually updated checklist (Bérnils, 2010; also see Wüster & Bérnils, 2011), triggering a proliferation of dual nomenclature for this medically important group of venomous snakes. This example illustrates how the output from AJH can proliferate and the harm that can potentially result from this dual nomenclature. It should also be clear that it is impossible to determine what to do with the resurrected

names without addressing the new names that were coined at the same time. This is a nomenclatural issue, requiring a complete review of the entire group's nomenclature and its inherent taxonomy to determine how to proceed.

8. In our opinion, the issue at hand is not a narrow question of whether the names proposed in AJH may technically be nomenclaturally available, but how the broader scientific community, and the ICZN specifically, should best address this type of open, repetitive disregard of time-honored nomenclatural and taxonomic practice. A firm and unequivocal decision on this case by the ICZN is necessary to safeguard the scientific integrity and global perception of the closely intertwined fields of zoological taxonomy and nomenclature. Neither the global scientific community nor the ICZN itself should be held hostage now or in the future by individuals adept at web-based data-mining and self-promotion, who circumvent the spirit of the Code, minimally attempt to adhere only to the Code's narrowest technical premises, and pre-empt those who work in compliance with both the Code's Recommendations and its Code of Ethics.

9. Governmental agencies, inter-governmental conventions, NGOs, and the global scientific and conservation communities depend on and value credible scientific and taxonomic work by the herpetological and wider taxonomic communities. Important in this regard is the expectation of reasonable nomenclatural stability and a precautionary approach to recommended taxonomic and nomenclatural changes. A decision in support of Case 3601 would implicitly sustain a dual nomenclature for many taxonomic groups of reptiles, and likely facilitate future chaos for additional taxonomic groups. The confusion stemming from such a dual nomenclature would cause many problems, ranging from legislative difficulties (e.g. during the development and enforcement of species management and conservation strategies or for trade regulations and quotas), to confusion over the identification and management of venomous species in a medical context (Williams et al., 2006).

10. We are not advocating that the practice of zoological taxonomy be restricted to scientific professionals—we welcome and encourage taxonomic and nomenclatural contributions from serious amateurs, naturalists, and biodiversity enthusiasts, as many of us are. However, such work needs to be original, acceptably published, and Code-compliant (including both the general Recommendations and the Code of Ethics), with a justified scientific underpinning. Additionally, we advocate strongly for quality-controlled peer-reviewed publishing as the only appropriate 'best practice' for new taxonomy and nomenclature (Turtle Taxonomy Working Group, 2007, 2014; Kaiser et al., 2013; Kaiser, 2014).

11. There are some similarities between the nomenclature presented in the AJH and that of Wells & Wellington (1983, 1985), the subject two decades ago of Case 2531 (BZN 44: 116–121) and several comments (e.g. BZN 45: 52–54, 145–153). This point has also been raised anecdotally on the ICZN online discussion forum and Taxacom, but we consider these cases to be dissimilar in important ways. The issues of concern in the Wells & Wellington papers were largely taxonomic and regionally focused (BZN 48: 337–338), whereas the issues with the AJH are primarily nomenclatural, ethical, and global. It has been argued in the past that the Wells & Wellington papers were also unethical and derived from substandard taxonomic practices (BZN 48: 337–338), but they were published prior to the existence of the Internet, and were not disseminated at the scale of AJH; nor did Wells & Wellington use their own self-edited

output as an opportunity to defame their critics in herpetology or professionals in other walks of life, as Hoser has done repeatedly, in breach of Appendix A:5 (e.g. Hoser, 2009, pp. 13, 16–19; Hoser, 2013b, pp. 12, 15), or to make political statements (e.g. Hoser, 2013a, p. 55) or to self-publicize business and other interests (e.g. Hoser, 2009, pp. 6, 9). In their ruling on Case 2531 the Commission concluded that the aim of that application would be best achieved by leaving the issue to taxonomic specialists to be settled through usage. In the present Case 3601, if the Commission were to consider the issue to be primarily taxonomic, this would have far-reaching destabilizing consequences for all of zoological nomenclature and taxonomy, consequences not readily resolved through usage.

12. Hoser's actions and abrasive comportment via AJH (citations in paragraph above) have created a highly contentious environment for zoological nomenclature and its intrinsic relationship to taxonomy. If the Commission upholds his request for validation of AJH issue 7, the greater scientific community and, importantly, future young scientists are likely to be misled into believing that output such as the AJH is an acceptable scientific medium for bringing knowledge to the public realm. Given also that Hoser's papers in AJH are, in our opinion, seriously flawed, unedited (evidenced by many uncorrected spelling errors), often potentially libellous and off-topic in content, and usually failing to present any measure of pertinent evidence to provide stability for the names they produce, their implicit endorsement by the Commission, if *Spracklandus* and AJH were to be validated, would in our opinion bring taxonomic science, zoological nomenclature, and the Code itself into unfortunate disrepute. Most herpetologists and many from other disciplines reject Hoser's output, as many have turned away from the ICZN online discussion forum, where Hoser has also been vocal and defamatory, including to ICZN Commissioners. In our opinion, this can in turn lead to an unwelcome erosion of the authority of the nomenclatural rules scientists have been following for over a century.

13. We believe Case 3601 represents a tipping-point in terms of where taxonomic science and its relationship with the Code might be headed. This relationship is currently being jeopardized by the actions of a single individual, which, if condoned or validated, are sure to further enable and facilitate others to follow. We therefore reiterate and support the proposal to suppress the entire run of AJH, as outlined in Proposals 9(1)b and 9(3) of Thomson (BZN 71: 134), inclusive of its most recent issues. Additionally, we urge that all scientists suspend the use of Hoser's nomenclature in order to avoid confusion. We contend that all taxa affected by new Hoser names contained in these issues of AJH be subject to prevailing usage under article 82.1 of the Code. Hence, no changes in order to use Hoser names should be formally made to their existing nomenclature while the Commission deliberates. This will prevent the names proposed in the AJH from coming into any further usage until such time that a Decision may be made. Should authors or editors feel a need to justify continued use of prevailing names for taxa affected by this proposal, they may cite Kaiser et al. (2013), Kaiser (2014), this comment, and Article 82.1 of the Code.

14. In summary, we contend that:

(1) the self-produced works by Raymond Hoser under the title of *Australasian Journal of Herpetology*, and the proliferation of names therein, are so contentious that they destabilize and cause confusion in the entire system of nomenclature, and undermine the scientific reputation and credibility of the discipline of taxonomy;

(2) the scope and reach of Hoser's nomenclatural output are made possible by relatively recent advances in internet communication, electronic publishing, and the use of social media, added to prior advances in desktop-publishing technologies. These tools are easily accessible across all biological fields, with concomitant risk of their deliberate misuse by some individuals. Thus, the nomenclatural issues we face in herpetology are already spreading and occurring in other branches of taxonomy and nomenclature. If left unchecked they will confound not merely nomenclatural stability in herpetology but taxonomic science as a whole;

(3) even though the Commission may be disinclined to rule on the basis of the recommended Code of Ethics contained in the Code, Hoser's demonstrated disregard for acceptable ethical practice is a key driver for many in both the herpetological community and other branches of zoology to categorically reject his writings and names, and support the retention of scientifically and ethically sound nomenclature. Without decisive pre-emptive action by the Commission, under Article 81.1 of the Code, the development of a dual system of nomenclature would appear inevitable, an outcome we regard as counter-productive in terms of Code-compliance.

15. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to declare the *Australasian Journal of Herpetology* (AJH) Volumes 1–24 unavailable for nomenclatural purposes; specifically pertaining to Case 3601, this would include issue 7 of AJH, thereby rendering the name *Spracklandus* unavailable;
- (2) to place on the Official Index of Rejected Works in Zoology issues 1–24 of the *Australasian Journal of Herpetology*, as ruled in (1) above;
- (3) to make a clear and decisive statement that the accepted scientific and ethical principles of zoological nomenclature, as recommended in the ICZN Code of Ethics, should be adhered to, and when evidence of failure to adhere to these principles leads to the matter being referred to the Commission for a decision, the Commission may apply and interpret the provisions of the Code of Ethics and determine whether it is appropriate to give a ruling.

These requests supersede those contained in three prior comments published by our co-authors (Thomson, BZN 71: 133; Wüster et al., BZN 71: 37–38; Kaiser, BZN 71: 30–35).

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Comment on *Allosaurus* Marsh, 1877 (Dinosauria, Theropoda): proposed conservation of usage by designation of a neotype for its type species *Allosaurus fragilis* Marsh, 1877

(Case 3506; see BZN 67: 53–56; 71–72; 178; 255–256; 332)

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We appreciate the supportive comments by Demirjian and by Loewen & Chure to designate USNM 4734 as the name bearer for the theropod dinosaur *Allosaurus fragilis* in place of the non-diagnostic YPM 1930. There are a few points raised that we wish to comment on:

1. Loewen and Chure are of the opinion that the premaxilla must belong to a different individual based on size comparison, although no supporting evidence is presented.

2. Marshall Felch, who collected USNM 4734, did so by what he called the ‘block method’, which he described in letters to Marsh, his employer: ‘The only way that I can do anything on the Carnivore (No 5) is by the block method. I think I can diagram and map so you will find how they go together.’ (June 15, 1884). ‘I take a strip at a time, with the seams – starting each strip from the outer edge (South Side) and working back as far as I think any bones of No. 5 are liable to be found which makes the breast of my strip now nearly 20 feet wide.’ (July 5, 1884). The removal of sandstone blocks along their joints (‘seams’ of Felch) was used from 1884 onwards. The pattern of the joints can be seen in plate 4 of Gilmore (1914), which shows the articulated *Stegosaurus* skeleton that was collected a few meters from USNM 4734 (skeleton 7). That Felch did collect USNM 4734 by the block method is stated in a letter ‘By the last of next week if we have good luck all can be shipped including No. 7 (*Allosaurus*) as we have it most all removed from its bed and part of the blocks dressed ready to group. . . but by great care we shall be able to get the blocks to match well – and all of the fragments saved and marked so as to locate without difficulty. . . [W]hat we found as far as the breaks show etc. – but we knew there will be found several perfect dorsals – ribs – one whole fore foot – all the cervicals in splendid condition – and some of the skull – how much we cannot tell as only a portion is exposed in a large block ’ (Oct. 28, 1884). Note that Felch clearly states that the skull parts were collected intact as a large block.

3. Charles Gilmore (1920, fig. 16) clearly refers the premaxilla to USNM 4734 in his detailed description of the skeleton. He did so because it was found associated with many of the other cranial bones during completion of the preparation of the skeleton at the National Museum of Natural History. Preparation of the specimen was begun in 1915: ‘the preparation of a fairly complete skeleton of *Allosaurus fragilis* was well under way.’ (Rathbun, 1916, p. 64). Work was completed the following year: ‘The skeleton of *Allosaurus fragilis*, mentioned in the last report, was entirely freed

from the matrix. . . The skull and jaws have been skillfully restored, articulated and mounted. . . ' (Rathbun, 1917, p. 58). That skull was figured by Gilmore (1920, fig. 1, pl. 3) and he was clear that the dentary used in the reconstruction belonged to a different specimen (USNM 8335). Gilmore in fact, was always clear when multiple specimens were used in a mount (e.g. *Camptosaurus* Gilmore 1912, p. 691; *Stegosaurus* Gilmore 1919, pp. 388–389). There is therefore no reason to doubt the association of the premaxilla with the rest of the skull.

4. Demirjian (2010) citing Chure's (2000) unpublished dissertation that the skull was reconstructed too short is correct. Chure (2000, p. 174–176) has described many of the problems with the skull, but we add several more that affect the visible relative size of the premaxilla and which may have misled Loewen & Chure (2010). First we note that the nasals have a distorted dorsal arch, which effectively shortens the snout. Some of this distortion was removed in the drawing of the left nasal by Gilmore (1920, fig. 9). This distortion is great enough that the real left nasal does not articulate properly with the left, real maxilla. The effect is that had the distal end of the dorsal or nasal ramus of the maxilla been complete, it would have projected into the upper portion of the antorbital fenestra. Despite the distortion of the nasal, the real, right premaxilla was articulated with the premaxillary process of the right nasal. To accommodate a shorter snout, the right maxilla was reconstructed a little shorter than the left, real one. In addition, so as not to reduce asymmetry between the left and right side of the reconstructed skull, almost 25% of the posterior half of the real premaxilla is hidden by plaster of the reconstructed maxilla, whereas on the left side, plaster of the reconstructed premaxilla overlaps a part of the real maxilla. In conclusion, we believe the evidence shows that the premaxilla associated with USNM 4734 does indeed belong to that specimen, and therefore is part of the proposed neotype for *Allosaurus fragilis*.

Additional References

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Comment on proposed replacement of the holotype of *Basilosaurus kochii* Reichenbach, 1847 (currently *Zygorhiza kochii*; Mammalia, Cetacea) by a neotype (Case 3611; see BZN 70: 103–107)

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Uhen (BZN 70: 103–107) requests designation of a neotype to replace the existing holotype of the late Eocene archaeocete *Basilosaurus kochii* Reichenbach, 1847. I ask the Commission to decline this request.

The history of *Basilosaurus kochii* Reichenbach, 1847, is outlined here, following Kellogg (1936) and others. Harlan (1834) named the genus *Basilosaurus* for ‘a vertebra of enormous dimensions’ from Louisiana in the United States but provided no species name. Owen (1839) recognized *Basilosaurus* to be a mammal rather than a reptile and proposed the replacement name *Zeuglodon*. Owen (1841) then named *Zeuglodon cetoides* based on dental remains from the Ocala Limestone Formation (upper Jackson Group strata of late Eocene, middle to late Priabonian age), in Clark County, Alabama (Kellogg, 1936; see also Jaramillo & Oboh-Ikuenobe, 1999). *Zeuglodon* is a junior synonym of *Basilosaurus*, and the name *Basilosaurus cetoides* is now widely used for the largest archaeocete from upper Jackson Group strata in Alabama. Synonyms of *Basilosaurus cetoides* include *Zeuglodon harlani* De Kay, 1842; *Hydrargos sillimanii* Koch, 1845; *Zeuglodon macrospondylus* Müller, 1849; and *Alabamornis gigantea* Abel, 1906. The type specimen of *Basilosaurus cetoides* is generally regarded as Philadelphia Academy of Natural Sciences 12944A, a lumbar or anterior caudal vertebral centrum measuring 36 cm in length and 18 cm in breadth (Kellogg, 1936; Uhen, 2013).

Basilosaurus kochii was named by Reichenbach (in Carus, 1847), based on a specimen from the Ocala Limestone (upper Jackson Group strata of late Eocene, middle to late Priabonian age) near Clarksville, Alabama. The type, variously called a ‘palate,’ ‘smaller palate,’ or ‘small skull,’ was illustrated by Reichenbach (in Carus, 1847, plate 2, figs. 3–4; see also Müller, 1849, plates 3–5). Reichenbach distinguished *Basilosaurus kochii* from *Basilosaurus cetoides* on the basis of its small size. Synonyms of *Zygorhiza kochii* include *Zeuglodon hydrarchus* Carus, 1849; and *Zeuglodon brachyspondylus minor* Müller (1851). True (1908) proposed the genus *Zygorhiza* for *Zeuglodon brachyspondylus minor* Müller. The type specimen of *Zygorhiza kochii* is in Museum für Naturkunde Berlin [MNB] Ma 43248 (previously 15324), a posterior cranium with occipital condyles that measure 11–12 cm across their outer margins (Müller, 1849, plate 3). Lumbar vertebral centra of *Zeuglodon brachyspondylus minor* are in the order of 6 cm long and 7 cm in breadth (Müller, 1849).

Finally, a medium-sized archaeocete, ‘*Zeuglodon*’ *brachyspondylus*, was named by Müller (1849) from Washington County, Alabama. This too came from upper Jackson Group strata of late Eocene, middle to late Priabonian age, and was distinguished by its large but anteroposteriorly short lumbar vertebrae. The type specimen designated by Gingerich (2007) is the lumbar vertebra illustrated in

Müller's (1849) plate 20, row 2, number 6, which Kellogg (1936) listed as an unnumbered MNB specimen that measures 19 cm in length and 19 cm in breadth. There is no cranium known for '*Zeuglodon*' *brachyspondylus*. Kellogg (1936) placed '*Zeuglodon*' *brachyspondylus* in the genus *Pontogeneus* based on *Pontogeneus priscus* Leidy (1852).

Pontogeneus priscus Leidy (1852) and *Cynthiacetus maxwelli* Uhen (2005) may be synonyms of *Zeuglodon brachyspondylus* or they may represent a fourth (and possibly fifth) upper Jackson Group lineage. Both are late Eocene, middle to late Priabonian in age. *Pontogeneus* is from the Jackson Formation of Caldwell Parish, Louisiana, and *Cynthiacetus* is from the Yazoo Clay of Hinds County, Mississippi. The one described centrum of a lumbar vertebra of *Cynthiacetus maxwelli* measures 8 cm in length and 12 cm in breadth (Uhen, 2005). The position of this vertebra in the lumbar column is uncertain, and it is missing anterior and posterior epiphyses.

Uhen (BZN 70: 103–107, paragraph 10) argues that the lack of a neotype for *Zygorhiza kochii* exacerbates ongoing difficulty in understanding cetacean diversity by preventing resolution of possible synonymy between *Zygorhiza kochii* and the related species *Dorudon serratus* Gibbes (1845) and *Chrysocetus healyorum* Uhen & Gingerich (2001). The type specimen of *Dorudon serratus* is from the Harleyville Formation of Berkeley County in eastern South Carolina, which is early late Eocene, early Priabonian in age (Uhen, 2004, 2013). It differs from all upper Jackson Group basilosaurids in being older geologically (early Priabonian versus middle to late Priabonian), and in coming from a different geographic province (Atlantic Coastal Plain versus Gulf Coast bordering the Gulf of Mexico). Further, *Dorudon serratus* is larger than *Zygorhiza kochii* and has more robust vertebrae and other comparable skeletal elements (Uhen, 2004, 2013). The type specimen of *Chrysocetus healyorum* is from the Pregnall Member of the Tupelo Bay Formation of Orangeburg County in eastern South Carolina, which is early late Eocene, early Priabonian in age (Uhen, 2013). Here again, it differs from all Jackson Group basilosaurids in being older geologically (early Priabonian versus middle to late Priabonian) and in coming from a different geographic province (Atlantic Coastal Plain versus Gulf Coast). Further, *Chrysocetus healyorum* is smaller than *Zygorhiza kochii* and it has more gracile vertebrae and other comparable skeletal elements (Uhen & Gingerich, 2001; Uhen, 2004, 2013).

Uhen (BZN 70: 103–107, paragraph 11) requests that the International Commission on Zoological Nomenclature use its plenary power to designate U. S. National Museum [USNM] specimen 11962 as a neotype of *Zygorhiza kochii* to replace MNB Ma 43248. The proposed neotype USNM 11962 comes from Choctaw County, Alabama, 50 km along strike northwest of the type locality, and from the same upper Jackson Group strata as the type. USNM 11962 is a more complete and better preserved specimen, however the holotype MNB Ma 43248 and the proposed neotype USNM 11962 both represent the same small basilosaurid species from upper Jackson Group strata. *Zygorhiza kochii*, like all species, should be thought of as a population of individual animals represented by a collection of known specimens. Replacement of the existing holotype by a neotype will not solve any pressing problem, nor will it change the collection of known specimens of *Zygorhiza kochii* in any way. USNM 11962 is available for comparison whether or not it is a neotype, and the existing holotype MNB 43428 identifies the collection of known specimens of